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VOLUME XLIX, No. 25 DECEMBER 4, 1952

THE JOURNAL OF PHILOSOPHY

A RE-EXAMINATION OF THE PROBLEM OF INDUCTION[1]

THOUGH the problem of induction is not found exclusively in modern philosophy, it is, in its present acute form, almost wholly a product of the epistemological investigations of a lengthy chain of philosophers beginning with John Locke. There is no need to offer proof that induction is intimately associated with epistemology. But this fact should be mentioned in justification for the present paper, which is, on the surface at least, much more concerned with epistemology than with induction as a special problem within the latter. I am here simply obeying the dictates of my own conviction that any kind of solution offered to the problem of induction will be contingent on modification of the basic epistemological premises that, in the history of modern empirical philosophy, have given birth to it. Nor is any apology needed for a preoccupation with Hume. It was Hume who set the problem in its modern and its most acute form, and it is to Hume, I believe, that we can most profitably return, for, as I shall argue, if we strictly interpret and consistently develop that aspect of his philosophy that most properly identifies him as an empiricist, we shall find ourselves facing an entirely different problem of induction than that which Hume himself posed.

The problem of induction, no doubt, can be formulated in many ways, each of which would offer us some special kind of insight into its nature. But whatever else it is, it surely is the problem of the transfer of belief. On the basis of the evidence (which, to be evidence, must be accepted with an attitude of belief) what am I justified in believing? It is a common observation that men bestow belief all too freely. They believe all sorts of things, sometimes without evidence at all, sometimes on what, to others, is obviously inadequate evidence. The problem of induction, then, is not just the problem of the transfer of belief, but that of justification of the transfer of belief. Those who seek an adequately grounded inductive method are asking for a touchstone that will discriminate,

[1] This paper was read, in slightly different form, in a symposium on induction at the annual meeting of the American Philosophical Association, Western Division, May 4, 1951. The present version has been much improved by the critical suggestions of Professor Sidney Zink of the University of Missouri.

among the beliefs that men actually do hold, between those that are "trustworthy" or "verified" or "reasonable" and those that are not. And such a discrimination certainly cannot be accomplished by scrutiny of a belief, quite by itself; it must, on the other hand, proceed by examination of the evidence on which the belief is held and of whatever bonds, be they logical or of some other nature, are asserted or assumed to bestow belief in the conclusion by transfer from belief in the evidence.

Hume's philosophy of knowledge encompasses two quite distinct doctrines that he intended to be complementary to one another, though that they are so is open to question. The first is his famous critique of all the standing beliefs of his day in the existence of objective bonds tying this experience together with that experience. The second is his psychological-descriptive account of the manner in which beliefs in such objective bonds originate. From the former we have inherited the problem of induction. To the latter many thinkers have looked for a solution to that problem. It may be worth our while, then, to bestow a bit of critical attention on the relation between these doctrines and to analyze Hume's notion of the nature and function of belief.

Hume's attack upon causality and substance, as devices employed by the rationalist to give objective sanction to the transfer of belief, is purely negative and destructive in effect. It does nothing by way of providing, or even suggesting, a substitute device for the transfer of belief. But, destructive though this attack is, it does not sweep an entirely clean path. Hume is questioning only the claims of *reason* to control the process of belief transfer. The *fact* of belief transfer he neither doubts nor disturbs. This *fact* becomes for him an object of interest which he does seek to analyze, descriptively. In other words, he is attacking what we *think* to be the grounds of our beliefs, not our beliefs as such. That is why he can advise us to fall back upon our instinctive beliefs, and act, in our ordinary life, as if we had never entertained the sceptical doubts his philosophy has forced upon us.

Belief, in its primitive sense, is traced back to a quality of "force or vivacity" (which he also calls "solidity," "firmness," and "steadiness") that accompanies those of our ideas Hume terms "impressions." This quality could be merely an idea added on to the ideas that make up the content of direct experience. But Hume expressly rejects this hypothesis on two counts:

> ***First,*** We have no abstract idea of existence, distinguishable and separable from the idea of particular objects. 'Tis impossible, therefore, that this idea of existence can be annex'd to the idea of any object, or form the difference betwixt a simple conception and belief. ***Secondly,*** the mind has the command

over all its ideas, and can separate, unite, mix and vary them, as it pleases; so that if belief consisted merely of a new idea, annex'd to the conception, it would be in a man's power to believe what he pleas'd.[2]

Since, then, it is not an idea, Hume concludes that it must be a "feeling" or "sentiment" "that depends not on the will, but must arise from certain determinate causes and principles of which we are not masters."[3] His reasons for this conclusion are clearly asserted and express one of those fundamental insights that justify Peirce's remark that Hume should be ranked higher as psychologist than as philosopher. It is simply that what we believe, we cannot simultaneously disbelieve. What comes before us in consciousness is accepted in a certain mode or degree of belief; its being what it is and its being accepted as fact or fancy are not separable aspects of it. This, of course, on the level of direct experience; but there remain those "certain determinate causes and principles of which we are not masters," and though in experience, as it comes to us, the feeling that constitutes belief is not separable from the content it accompanies, Hume does accomplish the separation in his positive psychology, through separation of those "determinate causes and principles" from the subject-matter on which they operate.

In Hume's psychology the conversion of an impression into an idea is the process of separating the feeling of "force or vivacity" from the content it accompanies, and all transfers of belief are processes of the contagious spreading of this feeling from impressions to associated ideas. Thus, "force or vivacity" does turn out to be separable and distinct, not as idea, or as a directly experienced something, but as something that is either itself a causative factor of experience, or an effect of such a causative factor. Suppose that we grant Hume the right to introduce into his psychology hypothetical or metaphysical entities that are not immediate products of his analysis of direct experience. Still, we would have to go further than this to justify his ultimate position; for if, as we have argued, such entities are required to accomplish the separation between a feeling of belief and its content, they are required not only to support the positive segment of his philosophy, but also to carry his program of negative analysis to the particular dénouement at which it arrives.

For Hume, the proof that ideas are "separate and distinct" is provided by an "experiment of the imagination" whereby they actually are separated, *i.e.*, imagined the one apart from the other. Suppose that we try such an experiment of our own, attempting to

[2] Appendix to the *Treatise*, Selby-Bigge Edition, London, 1896, pp. 623–624.
[3] *Ibid*, p. 624.

separate not idea from idea, but idea from the "sentiment of belief" with which it is entertained. At any moment one can always imagine that one is dreaming or can, in imagination, separate and re-combine distinguishable features of any present field of awareness. This blue book before me, for instance, I can conceive as being green. But are such experiments of the imagination performed on the objects of our belief or on something else? This blue book might be green, yes, but *is* it green? Certainly I would balk at such an assertion because it touches the object of my belief. Am I dreaming at the moment? I am willing to admit the possibility, but do I *believe* it? Abstract from the immediacy of a presentation, and belief can be separated from it. The question is, *can* we abstract from the immediacy of a presentation? Isn't its immediacy inseparable from it as a presentation? If the separation is made in imagination, what results is something of a wholly different nature from what we had to begin with. If one wishes to go directly to experience as a source for descriptions of experience, one should certainly note that we ordinarily know when we are having a direct experience and when we are indulging in imaginative experiments, and that we do not confuse the products of the latter with those of the former. Thus far we are in agreement with Hume, who explicitly recognizes the difference between what is directly experienced and what is imagined, and provides a pair of terms—"impression" and "idea"—to record the difference. But let us carry our analysis on, in the same vein, a bit beyond Hume.

If belief is not a detachable feeling, then what is presented in experience, infused with belief, is not a psychological something that has a degree of vivacity, but the object that we believe to be there. We cannot reduce the believed-in object to a complex of impressions and ideas without separation of this complex, as content, from belief as that which projects the content into a supposedly objective realm. It is for this reason that Hume's critical philosophy is not independent of the conclusions arrived at in his positive psychology. What is more, as we can see now, Hume employed this undeniably metaphysical concept of "certain determinate causes and principles" that control the occurrence and transfer of belief, to carry through the very analysis that emerged in a situation that made it seem reasonable and necessary to invoke those causes and principles! Without injection of these assumptions, experience would never have fallen apart into a collection of atomic sensa, and so no mysterious and inscrutable forces would have been needed to weld it together again into the semblance of objectivity and continuity in which it actually comes to us.

If belief, then, is not a detachable feeling, what is presented in

experience is not a sequence of detached atomic ideas, but a congeries of believed-in objects. That they are believed in is not a mere psychological fact, but testimony to their objectivity, since the only way in which any element of experience could offer up proof of its objectivity is through presentation in that particular mode we call "direct experience," which mode is identifiable solely by the fact that it enlists belief. When we say, "I saw it with my own eyes," "I heard it with my own ears," we are merely reiterating the fact of our belief. To see, to hear, to touch *is* to believe, and to believe is to assume that one could see or hear or touch, even if one is not doing so at the moment.

However, it would be very unwise to assume that such arguments as these will carry conviction among philosophers. After all, Hume was not exactly a fool, and he was as sensitive as any man—more sensitive than many of his followers have proved to be—to the appeal of direct experience. He has his reasons for rejecting the claims of naive experience, and we all know what they were. All of us do trust direct experience to begin with, and those of us who are at all observant find many times that it deceives us. Hume's critical philosophy is simply the ultimate product of a process that began with Locke and was intended to determine what portions of experience could properly be termed "direct"—*i.e.*, what, in experience, could be believed in without further ado. That this process should have culminated in the conclusion that there are *no* elements of experience, as it *really* is, that rightfully command our belief, is both startling and discouraging. But even more discouraging than this conclusion itself is the turn it gave to subsequent philosophy. When, failing to find a justification for belief in experience as he had conceived the latter, Hume turned to analysis of belief as a merely psychological phenomenon, he effectively negated the possibility of ever achieving any kind of critical control over the process of belief formation. Because there are no elements of experience that can rightfully command our belief, we must be content to take beliefs as they come; their incidence in experience is inscrutable, hence not open to rational criticism.

For Hume, of course, it is only the incidence of primary belief that is inscrutable. The transfer of belief, through the association of ideas, he regards as a phenomenon capable of analysis. Yet, in the end, what kind of yield can we expect from such an analysis? It is only by checking a belief that arises through association of ideas with its intended object that we can determine its adequacy, and it is only through a series of such checks that we can determine what modes of idea association are reliable or unreliable. But how do we go about looking for the object of a belief?

In experience as Hume defines it, there is no such object to be found. For him the object is created by belief. To check on a belief, then, we must accept a belief, uncritically; and, in the event that the check results in a rejection, on what grounds is this rejection to be made if *all* beliefs are held without rational grounds? An answer is available, to be sure: We should trust those beliefs that are grounded in impressions, *i.e.*, in direct experience. But this we perceive to be a circular answer when we recall that an impression has been defined as an element of experience that comes to us with that perculiar feeling that automatically enlists our belief! So it turns out that the only test of the adequacy of a belief is its force—or the degree to which we believe in it.

If Hume's theory of experience leads ever into such difficulties as these, we would do well to look around for another theory; yet we must hold in mind the paradoxes apparently resident in naive experience that led to the retreat of English empiricism into the impossible position occupied by Hume. Let us begin by refusing to separate belief from its object. This means that we will be committed to an acceptance of experience "as it comes"—*i.e.*, as we believe it to be. But won't all those paradoxes we have just been mentioning come flooding in upon us the moment such an admission is made. I suspect that they will always be with us, but not necessarily as insoluble. What we believe the objects of experience to be today is not quite what we believed them to be yesterday, and certainly quite different from what men believed them to be a hundred years ago. Beliefs undergo modifications. The question before us is, what is it that instigates this process of modification and is it rational, or can it be subjected to rational control? I would first call attention to the fact that we are not as badly off as Hume was, because what we believe in are structured objects that affect one another in a variety of ways. What instigates the modification of a belief is not a shift of feeling from one idea to another, but the failure of a believed-in object to behave as we believe it ought to behave. The expected behavior is not posed, baldly, against the observed behavior; on the contrary, both expectation and observation are embedded in a common matrix of beliefs. The expectation is based not merely on past repetitions of some linear series of experiences that have produced in us a habit. Rather, it grows out of our conception of the nature of the various objects that compose the situation from which the expectation emerges. The observation, too, is not the simple affair that some philosophers think it is. To compete with an expectation, an observation must be judged to be as objective as the situation from which the expectation was formed. For observations are of the behavior of objects, and our

conceptions of those objects enter into the act of observation exactly as they enter into the act of expectation. Thus the scientist is as cautious in acceptance of his observations as he is of the preconceived theories which these observations are to test, and he insists that observations be made by a technique which is, in effect, dictated by conceptions of the material he is observing. In consequence, scientifically acceptable observations are embedded in the same matrix of beliefs that controls the hypothetical development of expectations.

It is because the believed-in objects of experience have a structure, that reason has something to say about inductive thought. Logic can be applied to matters of fact only if facts are systematically interconnected in such fashion that some of them will exclude others. In Hume's world of pure phenomena there can be found no reason why any presentation should challenge any expectation or memory, because there is in this world no structure of experience within which one element can preëmpt the position occupied by another. The major problem of inductive *theory*, it seems to me, is that of determining the manner in which this background structure that makes it possible to apply logic to experience is to be set up, and what claim it has to our acceptance. Many thinkers have believed that logic itself provides such a structure. The most recent attempt to establish this thesis is that of Donald C. Williams in his little book, *The Ground of Induction*.[4] Professor Williams argues that probability theory in its strictly mathematical formulation can, without any additional assumptions, provide us with conclusions of a remarkably high degree of probability from premises that consist solely of observations of the distribution of characteristics in our experience. It is evident, from the reviews Mr. Williams' book received, that I am not the only one who doubts that such a thesis can be established. But my grounds for doubting may be a little different from those of others, so I would like to present them very briefly.

Logic, as such, does not determine a system. I think it is incontestable that we cannot think or communicate without applying the laws of logic to the subject-matter of our thought or discourse. But the laws of logic do not provide that subject-matter, nor do they determine what elements, within that subject-matter, shall be exclusive of what other elements. The systems we encounter in mathematics and the logical disciplines are all constructed by supplementing the laws of logic by sets of definitions that provide not only a subject-matter, but a catalogue, so to speak, of predicates and permissible and non-permissible combinations of

[4] Harvard University Press, 1947.

predicates that are applicable to this subject-matter. There was a time when it was thought that some such sets of definitions could be propounded that would generate not merely a *logical* system, but a system of empirical reals. This belief was usually defended by claims of "self-evidence" for the definitional set—*i.e.*, it was asserted to be self-evident that anything that *could* exist must be of such and such a nature. The history of philosophy makes it all too clear that there is no such set of "real" definitions capable of generating a logical system that holds the field alone, and recent proliferations of symbolic systems make it evident that the only thing that limits the number of variant formal systems is the fecundity of the human imagination. Whatever it is that makes one logical system more apt for the manipulation of a factual subject-matter than another, it doesn't seem to be any *a priori* insight into the logical structure of reality!

Let us pause here for a glance at the situation with which we are confronted. It is not the function of a theory of induction to prove that an objective world exists. This can be done only by what Hume termed the "sentiment of belief," and inductive thought has to deal with beliefs, not displace them or provide a substitute for them. But neither can it be the function of a theory of induction to justify inductive inferences by offering a set of real definitions that will bring experience under the critical supervision of logic. This task seems to belong to the man who is in the most intimate contact with the subject-matter to be defined—the scientist where the latter is scientific, and the philosopher only when he can make some claim to being an expert in the field in question. If no *a priori* insight into the formal structure of nature is to be had, then no comprehensive judgments can be passed on that structure from a "philosophical" perspective that abstracts from the detail of the techniques of acquiring knowledge. Yet there is another kind of problem inherent in inductive thought that the philosopher, as philosopher, perhaps can attack. As we have been arguing, logic, or reason, is not applicable to matters of fact unless fact has a structure; and the structure that gives reason its initial hold on fact is that stock of beliefs we all entertain about the objects of experience. But, as Hume has observed, experience of *structured* fact is modified by our beliefs, hence our beliefs are employed to provide evidence for themselves. There is nothing to ground a belief in but another belief, and since the criteria we employ to determine what are "reputable facts" are themselves derived from the body of beliefs we hold about nature, there is always the danger that the totality of our beliefs will fall into a backslapping, mutually congratulatory circle. I am suggesting that

the philosopher turn his attention to the abstract causes of the wholesale begging of empirical questions. Let it be frankly and flatly admitted that rational control of inductive thought is possible only on condition of the acceptance of preconceptions about the subject-matter of that thought; and that any such preconception *may* be question-begging. It may still be possible to minimize the begging of questions by the adoption of a set of precautionary rules governing the acceptance of preconceptions.

We hear a great deal nowadays about the self-corrective devices inherent in modern scientific method. That such devices exist, I do not doubt, but I do doubt that we understand fully how they operate. John Dewey has done as much as any man to contribute to our insight into these devices, and I suspect that his conception of the method of science as a democratic method is the key to the solution of our problem. Criticism must always proceed from grounds, but such grounds, as I have argued, cannot be supplied by the laws of logic alone. Logical criticism can operate only within a system; but if the latter is so "tight" as to exclude all subject-matter that does not fit neatly into its structure, it cannot be "tested by experience," since it will reject as irrelevant or as "illusory" all experience that offers a challenge to it. This is what a dogmatic metaphysics does in providing a set of rigid real definitions which fix the criteria of "acceptable evidence." The democracy of scientific method is resident in a certain looseness of the theory structure that antecedes any application of testing techniques. To my mind it is nonsense to talk about method in abstraction from theory, but it may make sense to talk about methods that are constructive of theory and critical even of the theories on which they are based and the theories that emerge from their application. It may make sense, but I don't think it is self-evident that it does, and what I would like to see is a theory of induction that would lay bare the abstract conditions under which such a theory-based, theory-constructing, theory-criticizing method is possible. I shall turn now to a brief consideration, not much more than a listing, of a few conditions that seem to me to be demanded by such a method.

First, an acceptable inductive method will have to be reflexive. It must be theory-based, yet it will be critical of the very theory on which it is based. Now any method that destroys its base is self-destructive, not self-corrective. That is the trouble with Hume's epistemology; it is destructive of all theory, as rational, and hence leaves no ground on which the applicability of logic to nature can be established. If a method is to be critical, but not destructive, of its base, it must be such that it can be applied by what might

be called "point application." That is, it must be capable of application here and there, now and then, to this and that specific problem, rather than to all problems at once. This is just another way of saying that the method must not be generalized overmuch; rather than a single technique, it must consist of a plurality of techniques that are adaptable to specific subject-matters and to specific aspects of those subject-matters. The aprioristic philosopher is inclined to think it a weakness of science that the latter employs many techniques in its many branches. I think this is one of the sources of its strength.

But, second, there must be a way of bringing specific conclusions, resulting from application of specific techniques to specific problems, to bear on the general matrix of theory within which these problems have been posed. If this were not done, science would splinter into innumerable investigation-sequences that would have no relevance to one another. Inter-relevance of the conclusions of specific investigations can be achieved through a common theory-base that guides the planning, execution, and interpretation of experiment, but, as we have been arguing, if this common theory-base is "tight" and "rigid" it will beg the very questions that are at issue by so dominating the selection of "relevant" evidence as to exclude anything that could serve as evidence against it. It is a commonplace of current philosophy of science that the "looseness" of science's theory-base is achieved by use of the method of hypothesis, which regards *every* proposition, general or particular, about scientific matter of fact as tentative. But like many commonplaces, this one does not, in my opinion, measure up to the claims made in its behalf. It does account for the fact that the theory-base is open to revision, but it does not adequately characterize the type of looseness that is to be found in science's theory-base. I could not possibly present here a critique of the hypothetical method as commonly conceived; but, by sketching several additions that I think have to be made to it, I may be able to suggest what I consider its weaknesses.

To account both for that looseness of articulation of its many branches of theory and of investigation that characterizes the scientific venture, and for that unity, sometimes actualized, sometimes merely promised or hoped for, that makes science something more than just the pronouncements of a community of dedicated individuals, we need to invoke certain general assumptions about the nature of the subject-matter of science. These, I think, are not, technically, hypotheses, for they are not, so far as I can see, subject to proof or disproof, though there can be derived from them propositions that can be empirically tested. The first of these

assumptions is that whatever it is that science is studying, it can be identified as the object of science's study without recourse to any of the hypotheses the scientist may entertain about it. In other words, there is a something, and not merely a hypothetical something, that serves as a subject for scientific propositions and as object for scientific investigations. What the scientist believes about this something might be wrong, but what he believes is a belief about *it*. Without the assumption that two beliefs can attach to a common object, there could be no incompatibility between beliefs. Furthermore, without the assumption that physical operations can be performed on a common object, there could be no incompatibility between the results of experiments. In short, the identity of the object, which is the starting point and the terminal point of inductive procedures, cannot be established by inductive procedure.

The second assumption is closely related to the first. In the absence of a wholly consistent, tightly integrated theory-base, science can achieve a loose sort of unity only by assuming that, no matter how divergent or how irrelevant its conclusions may seem in relation to one another, they have been derived from the study of common objects and are related to one another through those objects. The psychologist, the neurologist, the physiologist, the biochemist, not to speak of the economist, the anthropologist, and the historian, are all concerned with man as at least one of the objects falling within the domain of their subject-matter. But their interests attach to different facets of the complex object, man, and their techniques, accordingly, are so very different that it is perhaps impossible to justify one in terms of another. In other words, the unity of science is not dependent on an actual unity of theory, but is, rather, grounded in an assumed unity of subject-matter that will, eventually, yield a unity of theory. This assumption, too, cannot be established by inductive procedures, because it is too vague to be significantly tested. *Hypotheses* can be offered to account for the unity of the physiological and the psychological aspects of man, say, and these can be tested, but the rejection of any such hypothesis is never equivalent to a rejection of the assumed unity.

Do such assumptions as these help to prevent the unnecessary begging of questions? I have been, implicitly, arguing that they do, for they make possible an appeal beyond theory to an object whose unity and structure is not necessarily based on any particular theory, and which can be appealed to, therefore, to provide evidence for or against specific assertions deduced from this or that theory. But the word "assumption," as employed in the

above, is likely to lead us astray. What I intend it to refer to is not an attitude or belief that we purposely and consciously adopt for the sake of solving problems, but something we all naturally accept without question. If what was said earlier about belief is recalled—that belief is not a psychological feeling creative of objects believed in, but rather a token of the reality of the believed-in object—it can be seen that these general assumptions are concretely exemplified in the way experience comes to us. We experience objects, and the unity of the object is integral to the experience of it. The experienced object has a structure too, but the experienced structure is not necessarily complete. As a structure it offers a hold to theory, but as an incomplete structure it does not preëmpt this or that theory. The "appeal to experience" that is so necessary to the inductive method is an appeal to the structure of an experienced object, but it is also an appeal made in the name of some theory or other that would read that structure as a continuation of some larger structure not directly experienced. If the fact appealed to is to support the theory in whose name the appeal is made, the fact must not be constructed by the same activity that produced the theory.

Now it may be that the structure of our world of experience is mind-contributed. I do not see how this hypothesis could be logically refuted. But if this is the case, our inductive methods are without rational justification. And I do not see why we should accept such a supposition. Speaking for myself, I cannot entertain with an attitude of belief a theory that deprives the many beliefs I have of all rational justification.

W. DONALD OLIVER
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COMMENTS AND CRITICISM

A FOOTNOTE ON DESCARTES AND HUME[1]

In the February 28th issue of this JOURNAL, Mr. McLendon, in an article entitled "Has Russell Answered Hume?" argued, among other things, that Descartes and certain others "hold to a kind of geometric or 'linear' conception of inferring empirical conclusions from independently established premises" (p. 158), and that "*the traditional stringent Cartesian conception of knowledge* . . . demands that all knowledge be either intuitively self-evident (either

[1] Hiram J. McLendon, "Has Russell Answered Hume?" this JOURNAL, Vol. XLIX, No. 5, February 28, 1952, pp. 145–159.

to reason or to the senses) or derived by inference from intuitively self-evident truths of reason or the senses" (p. 147). By an argument based upon six theses, McLendon says that Hume showed that "this strict Cartesian epistemology . . . [has a] solipsistic outcome" (p. 148). Several of these points require critical attention.

In the first place, Descartes did not propose or adhere to the linear form of argument. The "conception of knowledge as a collection of intuitively self-evident propositions and propositions inferrible from self-evident propositions" (p. 148) was not held by Descartes, though it possibly was by other men who can be described as Cartesians. The source of this erroneous assumption may lie in the critique of scholastic logic, which was linear and syllogistic, and which therefore gave rise to the obvious question concerning the origin of the first premises of the chain of arguments. It would be easy for any person concerned with this problem, and indisposed to accept religious authority as the answer, to accept Descartes' "light of nature" as the modern substitute. But whatever the reason for the error, it is an error: this was not the function that Descartes himself ascribed to the "light of nature," and it does not appear that Descartes anywhere maintained the linear nature of inference. It is true that he proposed in his *Method* a strict order of investigation, but this was the order of inquiry from the most simple and obvious to the more complex and difficult, and not an order of logical deduction.

On the other hand, Descartes specifically argues in a non-linear fashion, when he says, towards the end of the *Discourse:*

> If some of the matters I deal with at the beginning of *Optics* and *Meteorology* should appear offensive, because I call them hypotheses and do not try to prove them, let the reader have the patience to read with attention, and I hope that he will be satisfied with the result. For it seems to me that the arguments follow one another in such a way that, just as the last principles are demonstrated by the first ones which are their causes, so these first ones are reciprocally demonstrated by the last which are their effects. And one must not suppose that I have here committed the fallacy which logicians call circular reasoning; for as experience makes most of the effects very certain, the causes from which I deduce them serve not so much to prove as to explain them—on the contrary, the truth of the hypotheses is proved by the actuality of the effects.

Let us pass now from the historicity of the attribution of a position to Descartes, to consider how McLendon has both Hume and Russell deduce from this position the sceptical outcome usually taken as Hume's philosophy. The argument runs as follows:

(1) Knowledge in general is impossible unless the principle of universal causation can be maintained. This is the *indispensability*

thesis, No. I. (Almost certainly, it would not be accepted by Descartes.)

(2) But the principle of universal causation cannot be maintained. This is the *groundlessness thesis*, No. V. For, according to the "traditional stringent Cartesian conception of knowledge," everything known must "be either intuitively self-evident (either to reason or to the senses) or derived by inference from intuitively self-evident truths of reason or the senses." We may note in passing that the conception of "self-evident to the senses" is quite alien to Descartes.

(3) But the principle of universal causation is not known by inference. This is the *thesis of the non-inferential character of the principle of causality*, No. IV. It is not inferred from what is intuitively self-evident to the senses, for the latter are sense-data, and universal conclusions can not be inferred from particular facts. And it is not inferred from facts plus "some general synthetic principle . . ., for it is this very universal principle of causality which is itself presupposed in any inference from particular facts to any generalization" (p. 147). The validity of this argument is questionable, for the causal principle may be necessary only to the derivation of synthetic universals from exclusively empirical facts, and may be further restricted to cases involving relationships of a causal nature, i.e., relationships of regular temporal sequence. It does not appear impossible that other types of synthetic principles may exist that can be derived from facts and analytic principles without the aid of the principle of universal causation. But perhaps this criticism should be directed against the first thesis instead of this one.

(4) The principle of universal causation is not intuitively self-evident to the senses. This is the *generality thesis*, No. III. For only particular propositions are given through the senses, and this principle is a general proposition.

(5) And the principle of universal causation is not intuitively self-evident to reason. This is the *non-analyticity thesis*, No. II. And here we encounter a gap in the argument: for let it be granted that the principle is "not an analytic proposition, that is, it is not one whose negation is a self-contradictory statement," which "is equivalent to saying that the principle is a synthetic one" (p. 147); nevertheless no argument has been offered for the conclusion that all propositions self-evident to reason are analytic. This thesis is not stated by McLendon, though it is an essential part of the argument, and one of its most questionable features—for reasons that must now be stated.

To begin with, Descartes' conception of the "light of nature"

involves the knowledge of the principles which God put alike in objective nature and in the minds of men: synthetic principles, not analytic; principles which determine the behavior of nature just as they are present to the thinking of man. The whole argument, Humean, Russellian, or McLendonian, thus is inapplicable to Descartes. But more, it would appear that it is inapplicable to all but empiricists. For surely those who propose that we have an insight, intuition, conscience, grace, right reason, common sense, or any other ability transcending sense, or that we are endowed at birth with innate ideas, do not intend to maintain that all this is insignificant. It is the extreme empiricist, trying to account for the possession by man of a few indisputable truths, who argues that these truths are analytic, verbal, and mere tautologies. Thus the argument outlined by McLendon would apply to a man such as Locke, and it would be reasonable to say that he has shown how the empiricism of Locke leads logically to the scepticism derived from it by Hume. But this is not a startlingly novel interpretation of the history of philosophy.

Let me add, in conclusion, and for the benefit of any person who might read this footnote without having read McLendon's article, that this footnote concerns only one aspect, and that a minor one, of his article.

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PROFESSOR HASEROT ON "BEAUTY AND INTERESTINGNESS"[1]

Professor Haserot's distinction is a useful antidote to the subjectivism rampant in so many current approaches to art. It is refreshing to consider the *object* we are *experiencing*, for this often leads to a clarification of the nature of the experience itself. While the author does not phrase his presentation in such terms, his remarks on expression and function underline this tendency toward "objectivity" in his thinking.

For him, all the characteristics of art which are irrelevant to it *as art* are covered by the term "interestingness"; he sees this as a psychological term which refers to the power of an object to stimulate other responses than those called forth by its aesthetic form. Form (which is what most people mean, or should mean, when they talk about "beauty") is a pattern of relationships giving a felt unity to a complex of perceptual elements; external reference, or any consideration of subject matter presented, is irrelevant to

[1] Francis S. Haserot, "Beauty and Interestingness," this JOURNAL, Vol. XLIX, No. 8, April 10, 1952, pp. 261–273.

aesthetic quality, which is totally summed up in the notion of form. When this felt unity

> attains a degree of completeness that precludes further alteration it is said to be perfect. . . . In a perfect instance of form . . . the parts are perfect as well as the whole. Everything is touched with a manifest inevitability. . . . If art is form it has an inner unity . . . but if it is interestingness, or dependent on interestingness, it separates into a thousand diverse processes with no essential, but with merely adventitious, relations among them. [Pp. 261 and 273.]

Here Haserot is in effect identifying his theory of form as a theory of the internal relatedness of all the elements of the object of art. Whatever is unnecessary, contingent, or externally related to the formal aspects of the object is irrelevant to the work as art and is merely "interesting." The key to the distinction between beauty and interestingness is, then, the doctrine of internal relations within the form of the art object; with this we must contrast the external relations of form to other aspects of art such as subject matter, expression, and function.

Such a distinction involves the following: If an element is internally related to the others together with which it constitutes a specific form, then its presence or absence changes both the form as a whole and the other elements related within the whole. An element not thus related is merely contingent; its presence or absence does not change the nature of the whole; it may be added or removed without affecting the whole. To see what this means in the case of aesthetic form, we may ask the following question: Given a case of aesthetic form, what is necessary to it in the sense of being internally related within it? Or in other words, when is the form no longer the "same" form; with what changes can we identify it as the same, and what changes can be allowed without making it "different"?

Consider a Bach sonata for unaccompanied violin. If the form of this work is all that is relevant to its aesthetic quality, it might just as well be played on the piano; the fact that the timbre of every note produced by the piano is different from that of the corresponding note on the violin should make no difference whatever to the *form* of the work. That is, the quality of the musical tones, which are the elements of the composition, must be considered irrelevant to the aesthetic quality of the object experienced if aesthetic quality is simply form. But this is not true; no competent critic would admit that the sonata could be as well played on the piano, regardless of the technical virtuosity of the pianist. He would insist that the sonata played on the piano is not "the sonata," regardless of the fact that its musical form is the same. The sonata played on the piano is a different work, because the sounding medium

changes the quality of the sounds produced; and it probably changes to some extent their relations to each other. Now, is the tone *quality* internally or externally related to the sonata as an aesthetic object, i.e., to its form? If it is internally related, then "form" is being used in a generally unfamiliar sense to include the performing medium; if it is not, then the medium is irrelevant to the work. But note what happens in the latter case: If the performing medium is irrelevant to form, then any technically adequate rendition would be as good as another, and we should have no basis for preference between instruments, between artists, between live and recorded performances, or between one key and another, so far as presentation of the work is concerned.

Or consider the frequent changes of medium through which a given work of art is often put today. It is not unusual for a best-selling novel, such as Betty Smith's *A Tree Grows in Brooklyn*, to be dramatized on Broadway, taken on the road by a different company, made into a movie, televised, or produced over the radio. Here are five different ways of presenting what one would suppose was the same "form": the plot of the novel. Current literary and dramatic criticism is full of discussions of the problems attendant upon converting a work from one of the media listed to another. Yet we are not interested in the media as such, but in the form of the work presented. The work may be changed to compensate for inadequacies of a second medium or to exploit possibilities which were not present in the first. Now, when is the plot no longer the "same" plot? Assuming that it had some aesthetic quality in the original, by what criteria do we judge whether it gains or loses in reproduction? Or, in the minimal case where no changes are required, is the better or worse impression a second showing makes on critics due strictly to factors which the psychology of aesthetics must treat; or *does* the difference "make a difference" aesthetically?

In short, if the form of the work of art—the plot of the drama or the musical structure of the sonata—is the only aspect to consider when we judge its aesthetic quality, we are faced with certain difficulties, of which a few have been described. Such a position seems to imply that all other factors, such as the performing medium, the performing artists, and the conditions of the performance, are external, psychological considerations irrelevant to the aesthetic quality of the work. This is a difficult conclusion to accept, not only because critics spend most of their time attending to such "psychological" aspects (for this may be a mistake on the critics' part) but because the problem arises as to whether a form remains substantially "the same" if certain aspects of its embodiment are changed. It is difficult if not impossible to define a form apart from its embodiment in a medium and its function when thus embodied. Even

if we agree that a "perfect" form is achieved in some specific instance, it has in effect no existence apart from its "materialization" in a given medium through the instrumentality of certain artists under specifiable conditions. And we have not even introduced the question of its relevance to human life.

We have confined our remarks to art works which demand performance or reproduction of some kind to be experienced. But take a Greco "View of Toledo." Modern methods of reproduction are such that it can be photographed in color with a good deal of accuracy. Now, do the reproductions (or a theoretically possible perfect oil copy) also reproduce the form, or not? That is, is it an irrelevant, external, and psychological approach which makes the original preferred to any type of copy? On Prof. Haserot's theory, I do not know the answer to this. If the copy does reproduce the form, then most discussion of art is concerned with non-essentials—and this may be true. If the copy does not reproduce the form, then how can one show this by the theory, except perhaps by redefining form in a wider and unfamiliar sense?

Thus, the question must be posed whether such a clear-cut distinction between necessary and irrelevant, internally and externally related aspects or elements of the work of art can be sustained in dealing with the objects themselves. If it could, many problems in aesthetics would be greatly simplified. But it appears doubtful that this is possible. Of course, if the distinction between internal and external relations were shown to be unclear it might not make the distinction between beauty and interestingness logically fallacious, but it would cast some doubt on its usefulness. This in turn calls into question the precision of the distinction between aesthetics and the psychology of art. If the internal and the external cannot be so clearly distinguished, then the disciplines which treat them are also not so clearly delineable, and the "fallacies" of content, expression, and function may deserve reëxamination.

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BOOK REVIEWS

Bertrand Russell's Construction of the External World. CHARLES A. FRITZ, JR. New York: The Humanities Press, Inc.; London: Routledge & Kegan Paul, Ltd., 1952. 243 pp. (International Library of Psychology, Philosophy and Scientific Method.) $4.50.

In this able and critical book, Professor Fritz traces the theme of logical constructionism in the writings of Russell. Quite cor-

rectly, he argues that constructionism does not build entities or objects but is essentially, as Russell conceives it, a method. "A logical construction is an analysis of a term which shows that the term is, in fact, an incomplete symbol and can be given a definition in use" (p. 218). Hence, saying of something that it is a logical construction is a brief way of saying that statements concerning that thing are equivalent to statements concerning other, empirical or anyhow supposedly more empirical things than are referred to by the original statement. Thus, saying "A table is a logical construction" is short for "Statements about a table are equivalent to statements about perceived events and unperceived events."

Fritz's thesis is that Russell developed constructionism in his very early work in mathematics, especially in regard to numbers, classes, and descriptions, then later, from 1914 on, enlarged the scope of application to include the entities of common sense, physics, and psychology. His major criticism is that Russell's use of the method in dealing with non-mathematical, inferred entities never solved the problem it was supposed to solve, namely, to render completely empirical our basic common-sense and scientific beliefs, since the translations effected by constructionism of our statements about material objects and point-instants, etc., if they were to do justice to these statements, had to include words that referred to unempirical, inferred entities.

Fritz's discussion of the construction of numbers, descriptions, and classes is accurate and illuminating. He presents the historical setting of Russell's problem here, namely, to formulate translations of statements supposedly about metaphysical and platonic entities into statements about less dubious things like classes of classes and eventually about propositional functions and their values or lack of values. On the whole Fritz is sympathetic to Russell's logistic program for mathematics and to the orthodox, Russellian treatment of so-called descriptive phrases. The interpretation of "The present King of France is wise" type of proposition as an existential proposition which *asserts* that there is one and only one present King of France, Fritz accepts as an adequate analysis.

Now, part of the reason his criticism of Russell's whole program of constructionism is as mild as it is, I think, is that he accepts the basic assumption of the program which is incorporated so clearly in Russell's theory of descriptions, viz., that meaning is a relation of correspondence between words or sentences and things or facts. Without this " 'Fido'-Fido" (Ryle) theory, the whole distinction between incomplete symbols and proper names and the

sinful doctrine that a word either means its object and hence is a proper name or does not mean an object and hence is an incomplete symbol dissolve. As Strawson has shown, Russell mixed up meaning and referring, which led him to his theory of descriptions according to which a proposition like "The present King of France is wise" is a false existential proposition, which it is not. If Strawson is right, which I think he is, what is fundamentally wrong with logical constructionism is not merely that Russell must assume unempirical entities in order to carry it out, but the whole theory of meaning upon which it is founded. The answer as to why constructionism is defunct in English philosophy today is not that it is a circular method, but that its roots lie in an unacceptable theory of meaning.

In his major works from *External World* through *The Analysis of Matter*, Fritz shows, Russell's preoccupation was the problem of the foundations of empirical knowledge and the attempt to reconcile "basic" knowledge with common sense and science. Cartesian doubt distinguished between basic beliefs and inferred beliefs, and logical constructionism offered the necessary translations of the latter into the former. But, as Fritz shows, these basic beliefs were arrived at through irrelevant psychological considerations or were stated unclearly or, more seriously, were insufficient in any adequate solution of Russell's problem of rendering science empirical. Supposing we do allow, Fritz suggests, that "brown patch occurring now" is a basic belief, certain in the sense that it can be completely verified (p. 104), how can we, even with logical constructionism, derive from this, without bringing into our premises beliefs about inferred, unempirical entities, the common-sense statement, "I see a brown cow"?

Fritz suggests that the minor role of constructionism in Russell's later work is due to the fact that he realized its inadequacy for dealing with this problem. In *Human Knowledge*, for example, Russell modifies his old problem of reconciling science and perception; here he asks, Given that science depends upon inferences to data, what would justify these inferences?, and his answer, of course, consists in the formulation of a set of postulates.

I have said nothing about the various discussions of logical atomism, metaphysical issues, whether or not constructionism is a new method, etc., in Fritz's book. They are all excellent. Indeed, the whole book is a most incisive, accurate, and well-presented account of this major thread in Russell's philosophy.

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The Problems of Value Theory. PAUL W. KURTZ. New York, 1952. v, 197 p. (Ph.D. thesis—Columbia University. Obtainable from the author. $4.75.)

This work, like all Gaul, is divided into three parts. In the first part, the author seeks to formulate the "nature" and problems of value; in the second he offers a critical discussion of several prominent naturalistic and emotive theories; in the third he expounds his views regarding something called "socio-cultural value."

Mr. Kurtz is aware of the abounding confusion within his chosen field. He notes the prevailing uncertainty and pessimism, which are all the more remarkable when one recalls that not so long ago it was widely supposed that a new philosophical star had been born under whose auspices there would be effected a complete reorganization and unification not only of most philosophical disciplines, but of the sciences of man as well. The situation, if the truth be told, is much worse than Mr. Kurtz himself appears to realize. For he still finds himself unembarrassed in talking about "value phenomena." He argues quite confidently that they must now be viewed in terms of the "socio-cultural field," and not as functions of that ghost in the sociological machine, the human individual. But some of his co-toilers in this vineyard have already begun seriously to wonder whether the expression "value phenomena" means anything at all, or whether its meaning is not so completely amorphous that the individual theorist (granting for the sake of argument that such a being exists) has no recourse save to stipulate what he himself will mean by the expression. Free stipulation, like free association, however, is of little interest, save to the clinical psychologists. Its philosophical significance is precisely *nil*—or if it isn't, then let someone spell out the reasons why.

It would be quite unfair, of course, to chastise Mr. Kurtz for talking about values, as though we all knew in a broad way what he is talking about, when so many of his elders (including myself) have set him such a horrible example. Still, there comes a time, at last, when one must protest against anyone's talking about values at all until he has first made quite plain what on earth it would be like to confront one of the slippery creatures in the flesh. It is not only philosophers who have sinned in this respect; the recent literature of sociology and social-psychology is chock full of gabble about "value situations," "value fields," and "value frame-work." "Good" and "ought" are vague and flexible enough, heaven knows; but "value," especially since it has been mauled by departments of social relations, has now lost all vestiges of bony structure. In such a predicament, it is the part of sanity to refuse flatly to acknowledge

that there are any preanalytic "value phenomena." Here is one expression—it is not alone—which has so tenuous a meaning that one cannot pretend to use it correctly or significantly without knowing its definition(s) in advance. Examples won't help much in this case. Indeed, the ancient and honorable philosophical technique of offering and examining instances and counter-instances can get no purchase, for the instances are too flabby to hold onto. Let us, by all means, go back to "ought" and "good" where there is at least something like a usage to which we can appeal in trying to gain our analytical bearings.

This is not intended to imply that there are no useful things said in this study. In the second part, where there are at least theories, if not usages, to cling to, Mr. Kurtz's discussion becomes somewhat more vertebrate. There are several telling, if largely unoriginal, criticisms of interest, affective, and emotive theories. Unfortunately there is little or no treatment of contemporary non-naturalistic theories, from which, in my judgment, Mr. Kurtz could learn a great deal. The more recent English moral philosophers are scarcely mentioned. This is a pity. They could help him to see more clearly what the problems are.

Turning to Mr. Kurtz's more constructive theme, I found myself from the outset involved in an ambiguity from which I was never able to disentangle myself. I won't pretend to say whose fault it was. Sometimes Mr. Kurtz seems desirous merely of enlarging the perspective from which the individual, with his complement of needs, attitudes, and "values" (are they a third thing?), is viewed. In this vein one takes him to be reiterating the well-known truths that man is (among other things) a social and cultural animal, and that man's various "functions" are "dependent upon" his transactions with his social environment. So understood, the dependence relation would by definition still leave the individual with a certain self to respect, and with "values," perhaps, which he could call his own. Certainly philosophers have not been wanting who have stressed this dependence. Some have even tried to trace it in some detail. At other times, however, Mr. Kurtz seems bent upon something rather more radical which really does "reduce" the individual (and his "values") to a vanishing-point in a field of culture. "Value," accordingly, is defined in terms of a "socio-cultural situation." Thus, for example, he informs us that "Not only is the genesis of attitudes dependent on the socio-cultural context, but even more important, the very *functioning, content,* and *substance,* of attitudes are largely socio-cultural" (p. 141, italics in text). Or again, "The 'personality' is the unique meeting place for the divergent activities of the society; where there is a split per-

sonality there is usually a contradictory set of social activities. The mind of the 'individual' organism and personality is what it does, and its issuance into the world in forms of activities (physical and symbolic motions, etc.) is in terms of the limits and possibilities of the ordered field. Thus the 'individual' is equivalent to the 'field' " (p. 144).

Now I do not wish to be captious. Mr. Kurtz is struggling with some difficult problems about which many writers before him have been no less vague and equivocal. By comparison with that much discussed collaborative volume, *Toward a Theory of Social Action*, for example, his writing is a model of firmness and lucidity. But much as I wish I did not, I do find in the end that he has presented a social and presumably a moral philosophy of the old "organistic" sort, according to which, as usual, the individual person becomes merely a cross-roads or point of intersection for social and cultural fields of force. When he talks about "sociological valuing," the ghost of old Durkheim (not to mention Bosanquet and Rousseau) returns again to haunt me. I had hoped that *that* ghost, at least, had been well laid. For my own part, I must simply say to Mr. Kurtz that I shall refuse to be regarded as a meeting place until he can assure me than someone will come to the meeting.

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PHILOSOPHY. Vol. XXVII, No. 103, Oct. 1952. *D. R. Grey:* Art in the *Republic. Philippa Foot:* The Philosopher's Defence of Morality. *A. R. White:* Conscience and Self-Love in Butler's Sermons. *H. D. Lewis:* What Is Theology? *H. J. N. Horsburgh:* Mr. Baier on Doing One's Duty.

PHILOSOPHY AND PHENOMENOLOGICAL RESEARCH. Vol. XIII, No. 1, Sept. 1952. I. A Symposium in Honor of C. J. Ducasse. *Arthur E. Murphy:* Ducasse's Theory of Appraisals. *Charles A. Baylis:* Intrinsic Goodness. *John W. Blyth:* What is a Sign? *Roderick M. Chisholm:* Ducasse's Theory of Properties and Qualities. *Lucius Garvin:* Ducasse's Carus Lectures. *Vincent Tomas:* Ducasse on Art and Its Appreciation. *Everett J. Nelson:* Categorial Interpretation of Experience. *Richard Taylor:* The Writings of Curt John Ducasse to December 31, 1951.—II. Discussions. *Donald C. Williams:* Professor Carnap's Philosophy of Probability. *F. C. Dommeyer:* Recent Literature in the History of Philosophy. *John Hershey:* Recent Latin-American Philosophy.

PHILOSOPHY OF SCIENCE. Vol. 19, No. 4, Oct. 1952. *Omar Khayyam Moore:* Nominal Definitions of 'Culture.' *Charles F. Hockett:* An Approach to the Quantification of Sematic Noise. *A. Bachem:* Heisenberg's Indeterminacy Principle and Life. *Howard Becker:* Science, Culture, and Society. *Adolf Grünbaum:* A Consistent Conception of the Extended Linear Continuum as an Aggregate of Unextended Elements. *John G. Kemeny* and *Paul Oppenheim:* Degree of Factual Support. *Edward H. Madden* and *Marian C. Madden:* Chauncey Wright and the Logic of Psychology. *George K. Plochmann:* Professor Henle on the Four Figures of Syllogism. *Henry Margenau:* Physics and Ontology.

NOTES AND NEWS

THE AMERICAN PHILOSOPHICAL ASSOCIATION, EASTERN DIVISION

FORTY-NINTH ANNUAL MEETING

DECEMBER 29–31, 1952

The annual meeting this year will be held at the downtown branch of the City College of New York, at the School of Business Administration, at the corner of Lexington Avenue and 23rd Street.

Members are reminded of the change of procedure this year with respect to all symposium sessions. Principal papers will not be read at the sessions. It will be assumed that members will, before the meetings, have studied these papers in the published volume, *Language, Science and Human Rights*. This volume is now ready and may be ordered direct from the University of Pennsylvania Press, 3436 Walnut Street, Philadelphia 4, Pennsylvania. The price of the volume is $2.50.

The program will be as follows:

MONDAY, DECEMBER 29

2:00 P.M. Symposium: "The Concept of Universal Human Rights." A. I. Melden and W. K. Frankena. Commentators: Felix S. Cohen and Lucius Garvin.

2:00 P.M. Joint symposium with the American Aesthetics Society, to be held at Sarah Lawrence College: "Symbolism in the Visual Arts." Rudolph Arnheim and Arthur Szathmary. Comments by Meyer Schapiro.

5:00 P.M. Business meeting.

8:00 P.M. Smoker.

TUESDAY, DECEMBER 30

9:15 A.M. Symposium: "What is a Rule of Language?" N. L. Wilson and R. M. Martin. Commentators: Alice Ambrose Lazerowitz and George Berry.

9:15 A.M. Papers on the History of Philosophy.

Bernard Peach: "The Indefinability and Simplicity of Rightness in Richard Price's 'Review of Morals'." Comments by Henry Aiken.

James W. Miller: "The Development of the Philosophy of Socrates." Comments by Robert M. Scoon.

E. A. Burtt: "A Comparison of the Basic Presuppositions of Western Philosophy Today with Those of 1900." Comments by Ledger Wood.

2:00 P.M. Symposium: "Problems of Concept and Theory Formation in the Social Sciences." Ernest Nagel and Carl Hempel. Commentators: Robert Merton and R. M. MacIver.

4:30 P.M. Tea. Host, Dr. Duell G. Gallagher, President, The City College of New York.

7:00 P.M. The Annual Banquet.

Memorial on the Centenary of Paul Carus, by C. I. Lewis.

Presidential Address: "The Philosophy of Natural Science and Comparative Law," by Filmer S. C. Northrop.

WEDNESDAY, DECEMBER 31

9:15 A.M. Symposium: "Phenomenalism and the Limits of Empiricism." Roderick Firth and Max Black. Commentators: Norman Malcolm and W. V. Quine.

9:15 A.M. Symposium: "The Concept of Expression with Respect to Art." Vincent A. Tomas and Douglas N. Morgan. Commentators: Monroe C. Beardsley and Bertram Morris.

GROUP MEETINGS

MONDAY, DECEMBER 29

10:00 A.M. The Peirce Society.

Morris Natanson: "The Concept of the Given in Peirce and Mead."

David Savan: "C. S. Peirce's Conception of Logic."

3:00 P.M. The Personalist Discussion Group.

Joseph D'Alfonso: "Personalism and the Mind-Body Problem."

4:00 P.M. The Association for Realistic Philosophy.

Robert C. Baldwin: "Preface to Ethical Realism."

TUESDAY, DECEMBER 30

12:30 P.M. Discussion Group for Creative Ethics.

Wayne A. R. Leys: "Ethics for Policy Decisions; Is It Ethics?"

We regret to learn of the death of Benedetto Croce at his home in Naples, November 20, 1952, at the age of eighty-six.

Current YALE books in Philosophy...

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by Paul Tillich

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by Karl Jaspers

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DESCARTES AND THE MODERN MIND

by Albert G. A. Balz

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by Max Hamburger

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